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VALENCE[1]

THE theory of valence is one of the most important theories of chemistry. Scarcely any other except the atomic theory, with which it is inseparably connected, has been so fruitful in results which have led to industrial applications and also to the development of chemical knowledge. In spite of these results, which no one can dispute, the theory is more or less in disrepute, especially among physical chemists and students and teachers of inorganic chemistry. In many of our elementary text-books structural formulas are used so sparingly that they make no impression on the student and in some of them they are not even mentioned.

This attitude is due, in part, to a reaction from the overemphasis given to the subject at a time when nearly all chemists were working on the structure of organic compounds. It is due, also, to confused and conflicting ideas about the philosophy of science.

Some have gone so far as to claim that speculations and hypotheses form no part of genuine science. To such persons science is only an orderly description of phenomena which we can see and handle, which we can weigh and measure and connect by mathematical processes. An attempt to acquire knowledge about atoms and electrons and molecules, so long as they remain beyond the direct cognizance of our senses, may be interesting but to followers of this school such attempts form no part of science.

To an organic chemist the achievements in the determination of the structure of carbon compounds demonstrate the falsity of such a claim. It may be remarked, in passing, that

[1] Address of the chairman and retiring vice-president of Section C—Chemistry, American Association for the Advancement of Science, Baltimore, December 27, 1918.

the philosophy of science referred to easily leads to the conclusion that the discovery of new facts is of supreme value in science and that one is doing good scientific work when he adds a few facts to our already unwieldy accumulation of knowledge, whether the facts discovered have any valuable relation to fundamental principles or not.

Another school of philosophers contends that the number of explanations which will fit any given set of natural phenomena is infinite, and that, for this reason, any explanation which we use, as for instance, the Copernican system or the atomic theory, is purely a product of our imagination and that it is hopeless ever to arrive at a system which shall actually correspond to the realities of the universe. This, if I understand him correctly, was the point of view held by Poincaré. It is only a step from this to the conclusion that there is no reality outside of our own minds, for, surely, if we can never attain to a knowledge of realities outside ourselves, for all practical purposes such realities do not exist.

A more true philosophy of science, as it seems to me, recognizes the intimate connection between speculation, hypothesis and theory on the one hand and the accurate study of phenomena on the other. Neither is complete or sufficient alone. Science advances most rapidly by what may be called a "cut-and-try" method. Speculation alone led to the useless dialectics of the school-men. A study of phenomena alone leads to an almost equally barren accumulation of facts for which we have no earthly use. It is inconceivable that chemistry, or indeed, that science as a whole could have made the progress that it has during the last century if Dalton, or some one else, had not given us the atomic hypothesis as a key for the study of chemical phenomena.

The subject of valence furnishes a particularly good illustration of the methods by which science advances. The positive achievements of the theory are so great that no one can doubt that there is some reality in the relations of atoms which corresponds to the theory. At the same time our knowledge is so vague and indefinite at many points that we must consider the theory as still very unsatisfactory and in need of further development.

Ernst von Meyer has pointed out with some truth that the theory of valence is implied in the Law of Multiple Proportions. A somewhat more definite approach was made when Graham demonstrated the polybasic character of phosphoric acid. His results were expressed, however, in the old dualistic formulas in which one, two or three molecules of water of hydration in the acid were considered as replaced by one, two or three molecules of a metallic oxide. When Liebig introduced the idea that acids are compounds of hydrogen the notion of polybasic acids became still more definite and the fact that an atom of antimony may replace three atoms of hydrogen while an atom of potassium replaces only one was given a clear statement. During the same period the discovery of the chloroacetics acids by Dumas and the development of the theory of types by Gerhardt and others gave greater precision to our knowledge of the replacement of one atom by another and it became evident that in such replacements one atom of oxygen may take the place of two atoms of chlorine.

Thus far the rudiments of the idea of valence had been developed only on the basis of the replacing power of different atoms. In 1852 Frankland went a step further and introduced the more exact conception of a definite, though variable, combining power for different atoms. Using the atomic weight 8 for oxygen he gives the formulas NO_3, NH_3, NI_3, NS_3; PO_3, PH_3, PCl_3; SbO_3, SbH_3, $SbCl_3$; AsO_3, AsH_3, and NO_5, NH_4O, NH_4I; PO_5, PH_4I, etc., to show that the elements nitrogen, phosphorus, arsenic and antimony combine with either three or five atoms of other elements. He also pointed out that when an atom of tin is combined with two ethyl groups in tin ethyl, $Sn(C_2H_5)_2$, it will take up only one atom of oxygen, giving the

compound, $Sn(C_2H_5)_2O$, while an atom of tin alone will combine with two atoms of oxygen to form stannic oxide, SnO_2.

A few years later Couper and Kekulé, quite independently of each other, developed clearly the idea that carbon compounds are held together in chains by attractions between the atoms and that the structure of the molecules of such compounds is directly dependent on the valence of the atoms of which they are composed.

In the same year, 1858, at Genoa, Cannizzaro revived the hypothesis of Avogadro and Ampère and gave such convincing evidence of its truth that it was soon accepted by the leading chemists of the world. This introduction of a correct system of atomic and molecular weights aided greatly in the very rapid development of structural organic chemistry. We can still imagine with what enthusiasm the chemists of that day seized the key to nature's mysteries which the doctrines of valence and of the linking of atoms had given them and applied them to the solution of problems of structure and of synthesis. Only a few years before the thought of definite, accurate knowledge of this kind would have seemed the dream of a hair-brained visionary.

Chemistry is primarily an experimental science. New theories make their way slowly and speculations which are not forced upon us by incontrovertible facts have met with little favor. At the time when the theory of valence made itself indispensable as a guide to the investigation of carbon compounds the older electrochemical theory had practically disappeared and no theory for the cause of the attraction between atoms received more than passing attention. It was tacitly assumed that some sort of attraction between atoms held them bound together but even such necessary terms as "single bonds" and "double bonds" or "linkages" were used with reserve by many chemists.

During the forty years following the publication of the papers by Couper and Kekulé the theory was amplified in only one important detail. The original theory considered only the sequence of atoms in compounds. While there may have been occasional thoughts about arrangements in space, chemists were very reticent in expressing them. In 1874, however, van't Hoff proposed an explanation of the relation between the structure of optically active compounds and the arrangement of their atoms, based on the fundamental proposition that four univalent atoms or groups combined with a given carbon atom are arranged symmetrically about the center of the atom. From the same starting point he postulated the supposition that two carbon atoms connected by a double union can not rotate independently about the points of union. The first hypothesis gave a satisfactory explanation for optically active compounds and it is impossible now for any one to question the fact that a compound which is optically active in solution must contain a central atom or group around which four or more atoms or groups are arranged in an asymmetric fashion. Incidentally it may be remarked that the discovery of compounds in which the asymmetric atom is nitrogen or sulfur or tin demonstrates that the principle of valence is general in its application and is not simply of value for carbon compounds.

The use of van't Hoff's principle in the explanation of the isomerism of such compounds as fumaric and maleic acids was equally successful.

In 1885 Bayer gave the following statement of the well-established principles used in explaining the structure of carbon compounds:

1. Carbon is usually quadrivalent.
2. The four valences are alike.
3. The valences are symmetrically directed in space from the center of the carbon atom.
4. Atoms attached to the four valences do not easily exchange places—van't Hoff's principle.
5. Carbon atoms may be united with one, two or three valences.
6. The compounds may be either open chains or rings.

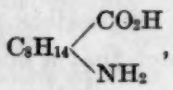
Baeyer proposed a seventh principle:

7. The directions in which the valences are exerted may be diverted from the normal angle of the tetrahedron, which is 109° 28′,

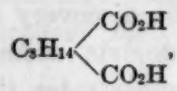
but the tension which results renders the compound less stable.

He explained in this manner the extreme instability of acetylene compounds and the ease with which additions are made to double unions; also, the instability of rings of three carbon atoms in comparison with those of four or five atoms. This so-called "tension theory" has been very suggestive and useful.

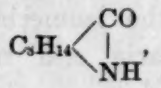
An interesting confirmation of the ease with which rings containing six atoms are formed and evidence that rings containing seven atoms are not so natural was discovered, almost by accident, by Mr. Potter and myself a few years ago. Aminocamphonanic acid, usually written,



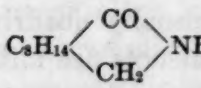
is a compound in which the carboxyl and amino groups are separated by three carbon atoms. It is derived from *d*-camphoric acid,



and we should expect it to have a right handed rotation for polarized light. Both the free acid and its anhydride,

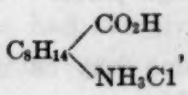


are, however, left-handed in rotation. The anhydride of its homologue, α-aminocampholic acid,

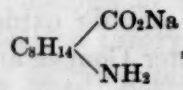


is also a lævo compound.

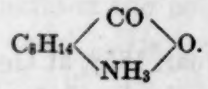
The hydrochloride of aminocamphonanic acid,



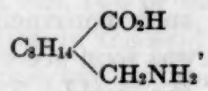
has the right-handed rotation which would be expected of the free acid. If we add to a solution of the hydrochloride one mol of sodium hydroxide, liberating the free acid, the rotation of the solution changes, from right-handed to left-handed. The addition of a second mol of sodium hydroxide, forming the sodium salt,



again causes a right-handed rotation in the solution. Similar relations were found for a number of allied compounds. The only simple explanation which is apparent is that the free acid has the cyclic structure,



The homologue,



on the other hand, is a dextro compound and both its hydrochloride and its sodium salt are right-handed, indicating that the free acid, which would require the formation of a ring of seven atoms to form a cyclic salt, does not form such a salt.

These relations seem to establish, also, the quadrivalence of nitrogen in ammonium salts. It seems impossible to reconcile this with Werner's idea that the fourth hydrogen of the ammonium group remains combined with the acid radical in such salts.

In 1899 Thiele proposed his theory of partial valences to account for the addition of bromine, hydrogen, or two other atoms or groups to the end carbon atoms in conjugated double unions, which have the structure $>C=CH-CH=C<$. The compound formed has the structure $>CX-CH=CH-CX<$. This relation always reminds me of two bar magnets in which the north pole of one is in contact with the south pole of the other $\boxed{N\ S|N\ S}$. At the center of such a system no attraction will be exerted but the free ends will exert their usual attraction. While this is an analogy rather than an explanation, and is also rather closely related to Thiele's idea, it seems to me better than that which he has expressed. If we attach any definite meaning of localized attraction to the term valence it is difficult to conceive of the valence as being divided, as seems to be implied in the term, "partial valence."

Werner in discussing this point, and also in discussing the Walden inversion and other topics, rejects the idea of localized attraction and prefers the notion of a general attraction of the atom as a whole. But a general attraction seems hard to reconcile with the fact that it is never satisfied by more or less than four univalent atoms or groups.

In 1887 Arrhenius proposed his theory of electrolytic dissociation, or, as it is better called, of ionization. The attention of chemists was once more directed to the intimate relation between electrical forces and chemical affinity. About ten years later J. J. Thomson demonstrated the existence of electrons as definite units of negative electricity having a mass eighteen hundred times smaller than that of the hydrogen atom. Almost at the same time the first discoveries of radioactive elements were made and as these were developed Rutherford brought out his theory of the disintegration of atoms. It soon became evident that atoms are not homogeneous, indivisible particles, as had been more or less tacitly supposed but that they must be complex aggregates of electrons and much more dense material with a positive charge. J. J. Thomson made an attempt to account for the properties of atoms by supposing a uniform positive sphere into which electrons found their way and distributed themselves in accordance with their attraction for the positive matter and their repulsion for each other. In contrast with this Bohr, Rutherford and others have assumed a very dense central nucleus, with a positive charge, about which electrons are in very rapid rotation. Experimental evidence based especially on the scattering of helium atoms which are shot through a thin film of gold or other metals has given very positive evidence in favor of atoms with a central nucleus. Some progress has been made in the discussion of the relation between the spectra of some of the elements and the revolving electrons of their atoms.

While physicists have been busy with studies of radioactivity and have reached some rather positive conclusions about atoms, which are certainly dynamic, rather than static, chemists have been busy with the applications of the new electro-chemistry in its relation to the electron theory. It is evident that in ionization a positive univalent ion has lost an electron and a negative ion has gained one. Chemists have usually been very reticent about the fate of these electrons when ions unite but it seems rather certain that some atoms still retain their position or negative character in their compounds. This was first definitely pointed out for nitrogen trichloride, in which the chlorine is positive, as contrasted with nitrosyl chloride, NOCl, in which it is clearly negative.

The evidence for the existence of positive chlorine in nitrogen trichloride and of negative chlorine in nitrosyl chloride is usually stated to be that the trichloride hydrolyzes to ammonia and hypochlorous acid while nitrosyl chloride hydrolyzes to nitrous acid and hydrochloric acid. Recent experiments, not yet published, have shown that dry ammonia and chlorine react with the formation of the trichloride and also that a dry solution of the trichloride reacts with dry hydrochloric acid to form ammonium chloride and chlorine. These facts seem to prove that the trichloride is formed by the addition of chlorine to ammonia, forming chloroammonium chloride, $NH_3Cl^+Cl^-$, followed by the splitting off of hydrochloric acid, and a repetition of the process till all of the hydrogen is gone. On the other hand, the decomposition of the trichloride by hydrochloric acid is caused by the addition of the acid, giving trichloroammonium chloride, $NCl_3H^+Cl^-$, followed by the splitting off of a positive and a negative atom of chlorine, as free chlorine. Under suitable conditions these reactions may be practically quantitative in either direction.

The close analogy between these reactions and those which take place in the substitution of chlorine for hydrogen in methane or in benzene is very striking and recalls Michael's theory that addition compounds are at first formed in such cases. The idea which is partly new is that such additions are always of two atoms or groups, one of which is

positive and the other negative. Also that atoms may form unstable compounds of higher valence when one of the new valences is positive and the other negative. The instability of such compounds is due to the fact that they contain two atoms or groups of opposite sign.

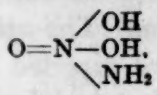
This is closely related to Abegg's contravalences, according to which the non-metallic elements, in particular, exhibit maximum positive and negative valences the sum of which is eight. Thus sulfur has a negative valence of two in hydrogen sulfide and a positive valence of six in sulfur trioxide and in sulfuric acid. But sulfur very readily assumes three negative valences, in such compounds as triethyl sulfonium iodide, $(C_2H_5)_3SI$, in which it has also assumed one positive valence. We may assume that methane, in a similar manner forms with chlorine the compound $CH_4Cl^+Cl^-$, which at once splits off hydrochloric acid.

It seems worth while to remark that we have much need of revising our conception of the non-metallic elements as being *negative* elements. This is based on the fact that the halogens when univalent form their most stable compounds as negative atoms and also because nitrogen, sulfur and similar elements form very stable negative groups, such as SO_4, NO_3, etc. We are a little apt to forget that oxygen forms the negative part of these groups and that nitrogen and sulfur are *positive* in them. Some apparent anomalies in the laws for the orientation of substituting groups in benzene derivatives are easily explained when these facts are kept in mind.

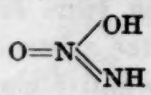
It will have been noticed that very much importance is attached, above, to additions and decompositions in which the two groups or atoms added or split off are of opposite sign. Nef, Michael and others have often emphasized the importance of reactions of this type but at a time when an accurate consideration of the positive or negative character of the atoms or groups involved was not in vogue.

Some of our ideas need revision, as it seems to me, in this regard. Let us take the commonly accepted formula of nitrous oxide,

The compound is formed by the decomposition of ammonium nitrate. This would almost certainly begin by the splitting off of hydrogen and NO_3, giving ammonia and nitric acid. Besides recombining to ammonium nitrate part of the ammonia would add itself to the nitric acid, giving



Such a compound would lose water, giving



first and then O=N≡N. Such a structure takes account of the probability that one nitrogen atom remains positive and the other negative throughout the series while the formula usually given implies that at least one valence of one nitrogen atom changes from negative to positive. The formula here proposed also seems more in accord with the ease with which nitrous oxide gives up its oxygen.

I can not take the time to consider the very interesting discussions of Falk and Nelson, of Fry and of Jones nor the valuable additions to our experimental knowledge made by the last two. As was remarked at the beginning, every important advance must combine experimental evidence with new explanations of natural phenomena.

G. N. Lewis has proposed the hypothesis that carbon compounds are not held together by polar valences because they do not readily ionize. It seems possible that this is true in some cases but it is difficult to believe that there is any very essential difference between the reaction of methyl iodide with silver nitrate and that of potassium iodide with the same reagent.

Werner distinguishes between primary valences which hold simple compounds together and also cause the binding of carbon atoms, and secondary valences, which are effective in the formation of complex, molecular compounds. Strong reasons have been given for thinking that the ammonium compounds are

genuine valence compounds and not molecular compounds, as Werner supposes them to be. If we assume rotating electrons, as we are almost compelled to do, molecules may well be held together by magnetic attractions and it seems possible that such attractions give rise to the secondary valences of Werner. He and his students have done magnificent work in the study of complex inorganic compounds, and some apparent contradictions between his ideas and those of organic chemists will doubtless be cleared up in the future.

To repeat, the most important recent advance in the theory of valence has been the interpretation of the theory in connection with the electron theory and a beginning toward the study of positive and negative atoms in organic compounds.

It is not very safe to prophesy, but I am impressed with the need of a more definite knowledge of the structure of atoms as a basis for a better understanding of valence. So much progress has been made in this direction during the past twenty years that further advance seems probable. Several different lines of study may be suggested, the coordination of which might lead to further knowledge.

The spectra of the elements must be intimately connected with the structure of the atoms. Professor Morley once suggested that the problem is something like that of a man who should endeavor to determine the mechanism of a grand piano with only sound waves to guide him. Yet the matter seems by no means hopeless and a beginning has already been made. It is noticeable that spectral lines are only given out by ionized gases and are possibly connected with the motion of valence electrons.

Certain forms of structure, especially those forms in which there are what Baeyer so aptly called *fliesende doppelte Bindunge,* absorb light of certain wave-lengths and give us colored compounds. This means that it is possible to calculate exactly the rate at which certain atoms, or parts of atoms, or of molecules vibrate.

The atomic numbers determined by X-ray spectra must be due to some more rapid and very fundamental sort of vibration which changes as the nucleus of the atom increases in weight from one element to another of the periodic system.

By means of X-rays it has been possible to determine the actual arrangement of atoms in crystals. The conclusion has been drawn from some of this work that the results do not agree with our customary conceptions of valence. I am inclined to think that the achievements of organic chemistry are not to be discarded so easily.

The Walden inversion has been interpreted by Fischer and Werner in terms of general attraction between atoms as opposed to localized attraction. The subject deserves careful study.

The evidence from radioactive disintegrations that helium atoms and electrons form essential parts of some atoms and very probably of all, is certainly important. The speculations of W. D. Harkins in this connection may be mentioned. The enormous quantity of energy liberated by the disintegration of an atom is certainly significant.

Three suggestions as to the functions of electrons in holding atoms together may be mentioned. One is that one or two electrons rotate about a line joining the centers of two positive nuclei, at a point midway between them. In ionization these electrons must remain with the negative ion. A second suggestion is that the magnetic effect produced by rotating electrons may cause an attraction between atoms. This is the magneton theory of Parsons and something of the same sort was suggested by Sir William Ramsay in the last paper which he published. It would also account for the non-polar combinations of G. N. Lewis. A third suggestion is that an electron may rotate about positive nuclei situated in two atoms. The velocity of electrons must be so much greater than ordinary motions of atoms that the atoms might be kept together in this way.

Whatever conclusions may finally be reached with regard to the structure of atoms it seems very certain that the structure will be found

to be dynamic rather than static. It is hard to conceive of a quiescent electron.

William Albert Noyes

SOME RECENT CONTRIBUTIONS TO THE PHYSICS OF THE AIR. II

BAROMETRIC FLUCTUATIONS

Another meteorological phenomenon that has been the subject of numerous investigations, is the pressure of the atmosphere. This pressure undergoes many changes, and the causes of some of them, such as its variations between summer and winter, and between ocean and continent, have long been known. But the causes of certain others, notably the great changes that accompany cyclones and anticyclones, have not yet been fully determined. Neither of these classes of changes, that is, those whose causes have long been known, and those whose causes still remain partially obscure, will be discussed here, though each is important and full of interest. But there are two other classes, namely, barometric ripples, and the pressure wave of semidiurnal period, that, because of their obvious interest to physicists, will be briefly considered.

Barometric "Ripples." — Small pressure changes having amplitudes usually of 0.1 mm. to 0.3 mm. and periods of 5 minutes to 10 minutes, and continuing for hours or even days together, are very common during cold weather. They are not greatly different in magnitude from the well known wind effects on the barometer, but obviously of different origin since their amplitude has no relation to the local wind velocity.

Their explanation appears to lie in the fact that whenever layers of air that differ in density at their interface flow over each other, long billows, analogous to water waves, and generated in the same way, are produced. If, now, the under layer is colder than the upper, as it is during the radiation or surface inversions of winter, and rather shallow, 100

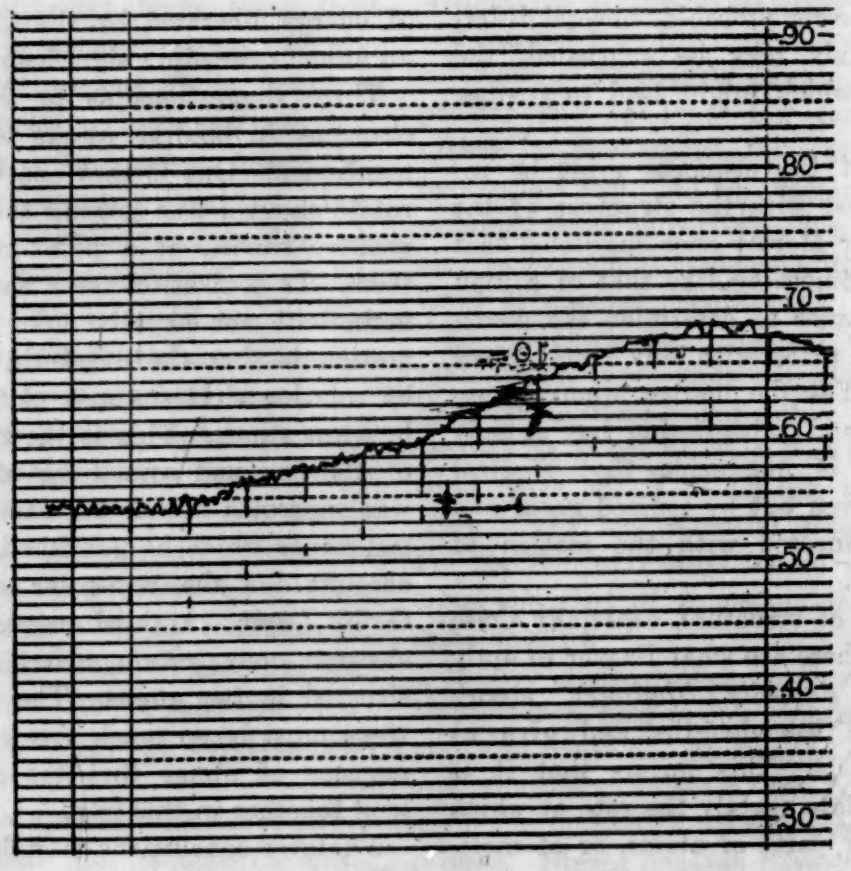


Fig. 4. Barometric Ripples.

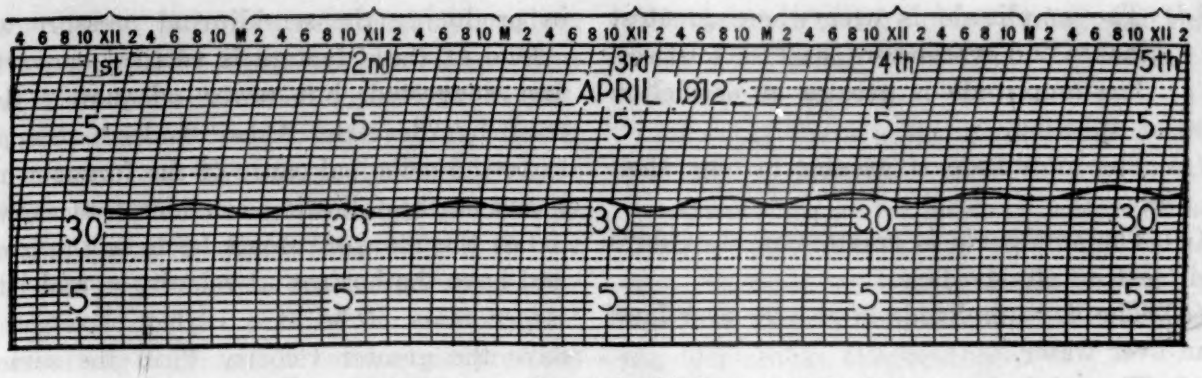


FIG. 5. Balogram, Grand Turk Island, West Indies.

meters to perhaps 500 meters thick, the passage of the air billows, like the passage of waves in shallow water, necessarily produces greater or less corresponding changes in the pressure on the bottom—changes that, as shown in Fig. 4, appear as a series of ripples in the record of a sensitive barograph.

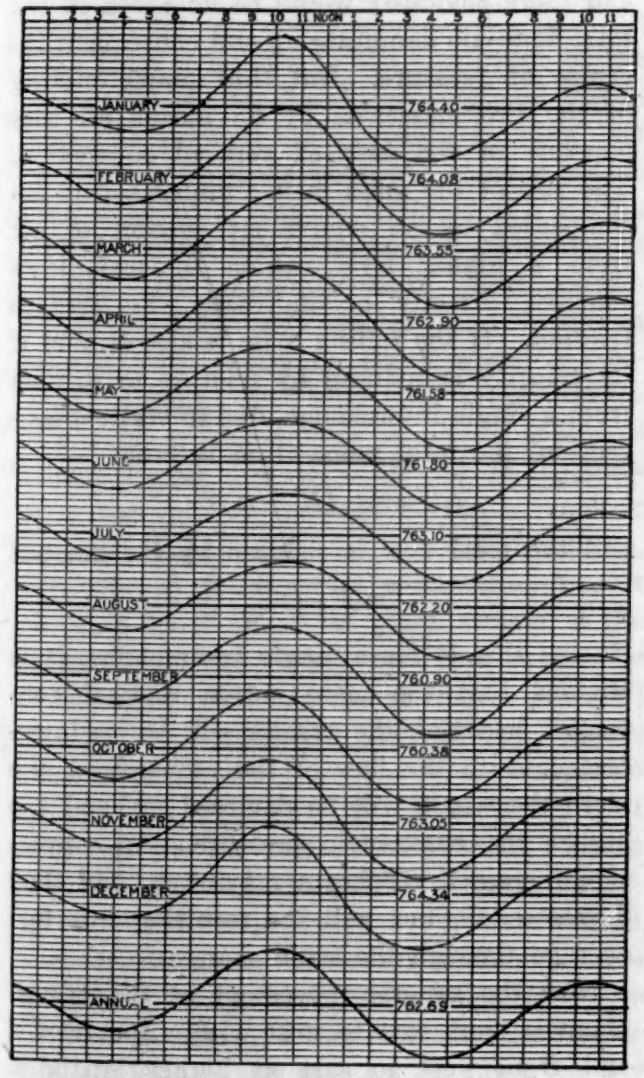


FIG. 6. Average daily barometric curves, Key West, Florida.

During the summer, when air billows rarely form near the surface, though frequently at greater altitudes, especially that of the cirrus cloud, barometric ripples do not appear. This, doubtless, is because wave disturbances in air, as in water, do not penetrate far beneath the wave level.

Semidiurnal Pressure Changes.—It has been known now for more than two and a half centuries that there are approximately regular daily variations in the height of the barometer that culminate in two maxima and two minima in the course of twenty-four hours. During particularly calm weather these fluctuations are conspicuous on the current barogram, as shown in Fig. 5, obtained at Grand Turk Island, West Indies, and always revealed by averages, no matter how masked by storm conditions, as shown by Fig. 6, based on records, covering fourteen years, at Key West, Fla.

Although, as stated, this phenomenon of the semidiurnal variation of the atmospheric pressure has been familiar almost since the invention of the mercurial barometer, and although repeatedly studied, its cause or causes remained until quite recently wholly unknown.

Some of the observed facts in regard to this twelve-hour cyclic change of pressure are:

(*a*) The maxima occur at, roughly, 10 o'clock A.M. and 10 o'clock P.M.

(*b*) The minima occur at, roughly, 4 o'clock A.M. and 4 o'clock P.M.

(*c*) The amplitude, when other things are substantially equal, varies with place approximately as the square of the cosine of the latitude.

(*d*) The amplitude is everywhere greatest on equinoxes and everywhere least on solstices.

(*e*) The amplitude is greater at perihelion than at aphelion.

(*f*) The amplitude is greater by day than by night.

(*g*) The amplitude is greatest on clear days and least on cloudy days.

(*h*) The day amplitude is greater over land than over water.

(*i*) The night amplitude is greater over oceans than over continents.

(*j*) Over the tropical Pacific Ocean the forenoon maximum is about 1 mm. above, and the afternoon minimum 1 mm. below the general average pressure.

Taken together these facts compel the conclusion that the daily cyclic pressure changes are somehow the results of temperature changes. Many efforts have been made to find just how the two are connected—how, for instance, a diurnal change in temperature can produce a semidiurnal change in pressure, but until recently without much success. To be sure, the diurnal temperature curve is not a simple sine curve of twenty-four hours period, and, like any other curve, can be closely duplicated by a series of superimposed sine curves of proper amplitudes and periods. But this is far from satisfying to the average physicist, and particularly so if he has not yet seen by what process the actual unanalyzed temperature changes can produce corresponding pressure variations in the open atmosphere. Of course it may be argued that there must always be a flow of air from the warmer to the colder regions, and, therefore, a pressure wave of diurnal period perpetually sweeping around the earth. Such a wave does indeed appear in the analyzed data. And as the temperature curve yields sine waves of twenty-four hours, twelve hours, eight hours and other periods, so, therefore, may the pressure curve also. But while this may be a portion of the story, it certainly is not the whole of it. There are other and important connections between temperature changes and pressure variations that must be considered; and these indeed seem to be the chief factors in producing the semidiurnal pressure wave.

One of these factors is vertical convection, first suggested, but never adequately developed, by Cleveland Abbe. Indeed, it appears to be the principal cause of the forenoon maximum as the following consideration shows:

Let the mass m of air be near the ground and have the horizontal velocity v, and let the larger mass M be at a higher elevation and have the greater velocity V in the same direction. If now these two masses should mingle in such manner as to be free from all disturbance, except their own mutual interference, the resulting final velocity, U, in the same direction, would be given by the equation

$$U = \frac{mv + MV}{m + M}$$

and there obviously would be no check in the total flow—no damming up and consequent increase of pressure. But this simple mixing of the two masses is by no means all that happens in the case of vertical convection. The rise of the mass m is simultaneously accompanied by an equivalent descent of air from a higher level, which in turn loses velocity, directly or indirectly, by surface friction. If the falling mass is also m, and if its velocity is reduced by friction to v, then from a single interchange, due to vertical convection, the total momentum becomes

$$2mv + (M - m)V$$

and the total flow is reduced by the amount

$$m(V - v).$$

But as this is for a single interchange, it is obvious that the more active vertical convection becomes, the greater will be its interference with the flow of the atmosphere, the more the winds will be dammed up, and the higher the barometric pressure. As convection increases, reaches a maximum, and then decreases, so, too, will the resulting interference go through the same changes.

Now the general movement of the atmosphere is from east to west within the tropics and from west to east at higher latitudes. Therefore in either case such damming up

of the air as vertical convection may produce will be essentially along meridians, and thus a function of the time of day. But, in general, convection increases most rapidly during the forenoon, say eight to nine o'clock, is most active at ten to eleven o'clock, and reaches its greatest elevation about four o'clock in the afternoon. Hence the damming up of the atmosphere, due to vertical convection, and the resulting increase of barometric pressure must increase most rapidly during the forenoon, and come to a maximum about ten o'clock. After this the convectional interference decreases, while at the same time the amount of air in a vertical column of fixed cross-section diminishes as a result of expansion and overflow, until at about four o'clock in the afternoon the barometric pressure has, as a result of this overflow, reached a minimum.

To form some idea of the magnitude of the barometric change due to convectional turbulence, consider the atmosphere between two parallels of latitude near the equator. This limited quantity may be regarded as a stream flowing around the earth, having its minimum velocity and maximum depth where convectional interference is greatest, and maximum velocity with minimum depth where convection is absent. And since the linear velocity of a point on the equator is approximately 28 kilometers per minute, while during the forenoon the rate of increase of the barometric pressure at the same place is, roughly, 0.2 mm. per hour, it follows that a damming up, or check in the flow, of the given stream at the rate of 0.44 kilometers per hour would be sufficient of itself to account for the observed rise in the barometer. But if the average velocity of the wind, or flow of the stream in question, is 10 meters per second, which it may well be, the rate of decrease in velocity requisite for the given rate of pressure increase could be produced by having only 1 part in 80 of the whole superincumbent atmosphere brought to rest per hour, or the equivalent thereof, an amount that perhaps is reasonable. At any rate, the assumed velocity decrease is of the same order of magnitude as that observed to take place during, and as the result of, diurnal convection.

It appears then:

(*a*) That the afternoon minimum is caused essentially by overflow from the region where the atmosphere is warmest, or better, perhaps, from the meridian along which the temperature increase has been greatest, toward that meridian along which there has been the greatest decrease in temperature.

(*b*) That vertical convection interferes with the free horizontal flow of the atmosphere and to that extent dams it up and correspondingly increases the barometric pressure; also, that the time of this interference agrees with the forenoon changes of the barometer, and that its magnitude is of about the proper order to account for the forenoon barometric maximum.

The afternoon barometric minimum and the forenoon maximum, therefore, are to be regarded as effects of temperature increase; the minimum as due to expansion and consequent overflow; the maximum as caused by vertical convection and consequent interference with the free circulation of the atmosphere.

The forced afternoon minimum would occur in an otherwise stagnant atmosphere, and substantially as at present; but not so with the forced forenoon maximum, since the interference or damming effect depends upon a flow or circulation of the atmosphere, parallel roughly to the equator.

It remains now to account for the night ten o'clock maximum and four o'clock minimum, both of which appear to depend upon the natural or free vibration of the atmosphere as a whole.

This subject has been discussed by several mathematical physicists of great eminence. The latest and most complete of these discussions is by Lamb, who concludes:

> Without pressing too far conclusions based on the hypothesis of an atmosphere uniform over the earth, and approximately in convective equilibrium, we may, I think, at least assert the existence of a free oscillation of the earth's atmosphere, of "semidiurnal" type, with a period not very dif-

ferent from, but probably somewhat less than, twelve mean solar hours.

Hence any cause of pressure change, having a semidiurnal period or approximately so, would, if of sufficient magnitude and proper phase, account for the twelve-hour barometric curve. All that is needed, apparently, to give the semidiurnal pressure curve is a pressure impulse of the same period, twelve hours, as that of the free vibration of the atmosphere as a whole. And this is furnished by the forced forenoon barometric maximum, followed six hours later at the same place by the forced afternoon barometric minimum. In other words, taken together, the forenoon and afternoon forced disturbances appear to occur with the proper time-interval necessary to set up and maintain the twelve-hour free vibrations of the atmosphere.

The course of events at each locality appears to be substantially as follows:

1. A forced forenoon compression of the atmosphere, followed by its equally forced afternoon expansion, the two together forming one complete barometric wave, with a ten o'clock maximum and a four o'clock minimum, in harmony with the free vibration of the entire atmospheric shell.

2. Non-disturbance through the night or during the time of a single free vibration.

3. Repetition the following day of the forced disturbance in synchronism with, and therefore at such time as to reenforce, the free vibrations.

The series of disturbances is continuous, forced by day and free by night, but the resulting amplitudes of the barometric changes are limited, through friction and through the absence of perfect synchronism, to comparatively small values. Each point upon the atmospheric shell receives at every alternate swing a forced impulse in phase with the free vibration, and therefore at such time and in such manner as indefinitely to maintain the vibrations of the atmosphere as a whole.

The forenoon maximum and the afternoon minimum are primary disturbances equally forced but in different ways by the daily increase of temperature, while the evening maximum and the morning minimum are secondary disturbances caused by the joint action of the forced primaries through the twelve-hour free vibration of the atmosphere. In short, the semidiurnal swing of the barometer is a result of merely fortuitous circumstances—of the fact that the mass of the atmosphere happens to be such that the period of its free vibration is approximately just one half that of the earth's rotation.

ATMOSPHERIC ELECTRICAL PHENOMENA

The selected contributions to the physics of the air just reviewed belong to the domains of mechanics and thermodynamics. But there have also been recent contributions to other branches of the subject, especially to atmospheric electricity, among them,

(*a*) The discovery in 1900 by C. T. R. Wilson and, also, by H. Geitel of spontaneous ionization in the atmosphere.

(*b*) The discovery in 1902 independently by Rutherford and Cook, and by McLennon and Burton, of a penetrating radiation in the lower atmosphere, presumably from radioactive substances near the surface of the earth.

(*c*) The discovery in 1905 by Langevin of slow moving or large ions in the atmosphere.

(*d*) The discovery by Simpson in 1908 and 1909 that the electric charge on thunderstorm rain, and precipitation generally, is prevailingly positive.

(*e*) The discovery in 1908, also by Simpson, of the probable origin of the electric charge of the thunderstorm.

(*f*) The discovery by Kolhörster in 1914 of an extremely hard or penetrating radiation in the atmosphere that seems to be of extra terrestrial origin.

Each of these several discoveries has its own peculiar interest, but the origin of the electric charge of the thunderstorm involves more meteorological phenomena than does any of the others, and, therefore, it is selected for further remarks.

Many have supposed that, whatever the genesis of the thunderstorm, the lightning, at least, is a product or manifestation of the free electricity always present in the atmos-

phere—normal atmospheric electricity. Observations, however, seem definitely to exclude this assumption. Thus, while the difference in electrical potential between the surface of the earth and a point at constant elevation is, roughly, the same at all parts of the world, the number and intensity of thunderstorms vary greatly from place to place. Further, while the potential gradient at any given place is greatest in winter, the number of thunderstorms is most frequent in summer, and while the gradient in the lower layer of the atmosphere, at many places, usually is greatest from 8 to 10 o'clock, both morning and evening, and least at 2 to 3 o'clock P.M. and 3 to 4 o'clock A.M., no closely analogous relations hold for the thunderstorm.

But how, then, is the great amount of electricity incident to a thunderstorm generated? Fortunately an answer to this question based on careful experiments and numerous observations, and that greatly aids our understanding of the interrelations between the various thunderstorm phenomena, has been given by Dr. G. C. Simpson, of the Indian Meteorological Department.

The chief conclusions drawn by Simpson from his observational data, and supported by numerous subsequent observations by other persons at widely separated places, are:

(*a*) That the charge on thunderstorm rain, amounting often to 5 to 10 electrostatic units per cubic centimeter, usually is positive.

(*b*) That, on the whole, the quantity of positive electricity brought down is more than three times greater than the negative.

While these observations were being secured a number of well-devised experiments were made to determine the electrical effects of each obvious process that takes place in the thunderstorm.

Freezing and thawing, air friction, and other things were tried, but none produced any electrification. Finally, on allowing drops of distilled water to fall through a vertical blast of air of sufficient strength to produce some spray, positive and important results were found, showing: (1) That breaking of drops of water is accompanied by the production of both positive and negative ions. (2) That three times as many negative ions as positive ions are released.

Now, a strong upward current of air is one of the most conspicuous features of the thunderstorm. It is always evident in the turbulent cauliflower heads of the cumulus cloud—the parent, presumably, of all thunderstorms. Besides, its inference is compelled by the occurence of hail, a frequent thunderstorm phenomenon, whose formation requires the carrying of raindrops and the growing hailstones repeatedly to cold and therefore high, altitudes. And from the existence of hail it is further inferred that an updraft of at least 8 meters per second must often occur within the body of the storm, since, as experiment shows, air of normal density must have approximately this upward velocity to support the larger drops, those of 4 mm. or more in diameter, and, because of its greater weight, even a stronger updraft to support the average hailstone.

Experiment also shows that raindrops of whatever size can not fall through air of normal density whose upward velocity is greater than about 8 meters per second, nor themselves fall with greater velocity through still air; that drops large enough, 4.5 mm. in diameter and up, if kept intact, to attain through the action of gravity a greater velocity than 8 meters per second with reference to the air, whether still or in motion, are so blown to pieces that the increased ratio of supporting area to total mass causes the resulting spray to be carried aloft or, at least, left behind, together with, of course, all original smaller drops. Clearly, then, the updrafts within a cumulus cloud frequently must be strong and therefore break up at about the same level, that of maximum rain accumulation, innumerable drops which, through coalescence, have grown beyond the critical size; and thereby, according to Simpson's experiments produce electrical separation within the cloud itself. Obviously, under the turmoil of a thunderstorm, such drops may be forced

through the cycle of union (facilitated by any charges they may carry) and division, of coalescence and disruption, from one to many times, with the formation on each at every disruption again *according to experiment*, of a correspondingly increased electrical charge. The turmoil compels mechanical contact between the drops, whereupon the disruptive equalization of their electrical potentials breaks down their surface tensions and insures coalescence. Hence, once started, the electricity of a thunderstorm rapidly grows to a considerable maximum.

After a time the larger drops reach, here and there, places below which the updraft is small—the air can not be rushing up everywhere—and then fall as positively charged rain, because of the processes just explained. The negative electrons, in the meantime, are carried up into the higher portions of the cumulus, where they unite with the cloud particles and thereby facilitate their coalescence into negatively charged drops. Hence the heavy rain of a thunderstorm should be positively charged, as it almost always is, and the gentler portions negatively charged, which also very frequently is the case.

Such in brief is Dr. Simpson's theory of the origin of the electricity in thunderstorms, a theory that fully accounts for the facts of observation and in turn is itself abundantly supported by laboratory tests and imitative experiments.

. . . . . .

The foregoing are only a selected few of the many recent contributions to the physics of the air, but they are sufficient, it is hoped, to show that meteorology is indeed a progressive branch of physics, and one eminently suitable to every type of scientific talent. The close observer, the clever experimentalist, and the keen analyst all can find in the phenomena of the atmosphere inexhaustible material and endless opportunities. But in science opportunity is only a synonym for duty, and of all words duty is the noblest.

W. J. Humphreys

U. S. Weather Bureau

SCIENTIFIC EVENTS

THE HISTORY OF MEDIEVAL INSTITUTIONS

Professor David Eugene Smith, of Teachers College, Columbia University, writes:

The Société de Scolastique Médiévale, founded by M. Francois Picavet, professor in and secretary of the Collège de France, was changed into the Société d' Histoire Générale et Comparée des Philosophies Médiévales in 1906. This society is interested in the study of the history of dogma and religion, the history of law, the history of letters, the history of philosophy, and the history of science. Monographs have been issued in all these several lines and others are in the course of preparation.

It may seem that this is not an opportune moment for scholars to be considering such a line of work. Upon this point a letter from M. Picavet, written just before the armistice, has this to say: "En ce moment ou nous ne pouvons, en raison de notre âge, que faire des voeux, pour les combattants ou venir en aide aux prisonniers, aux soldats et aux évacués des pays envahis, j'ai pensé que nous pourrions nous rendre utiles en préparant les moyens de nous suffire sur le terrain scientifique et universitaire, entre nous gens de l'entente et amis de l'entente."

Few scholars have been called upon to make a greater sacrifice in this war than M. Picavet, and his determination to continue his great work in the field in which he has done so much will doubtless appeal to all scholars in this country as in Europe.

M. Picavet would be glad to hear from American scholars who are interested in the work of the society and to have their names enrolled as sympathetic with its work. If they should later become so interested as to contribute in any way to the support of the society, this would be a welcome decision; but this is not the immediate purpose. It would be a helpful act if those interested in this line of work were to write to M. François Picavet, Collège de France, Paris, expressing their interest in the society.

If America could in some way secure an endowment of $24,000 for maintaining for ten years the chair which M. Picavet fills with such distinction in the Collège de France, a great impetus would thereby be given to this work.

ACTIVITY OF KILAUEA VOLCANO

Professor Vaughan MacCaughey, of the College of Hawaii, Honolulu, writes under date of January 22:

Within the past few days word has been received from Mr. L. W. De Vis-Norton, at Kilauea Volcano, of the remarkable activity of this famous crater. He states that "tremendous changes are in progress at Kilauea, and there is no indication whatsoever of any cessation of the monumental rising of the entire vast lava column. . . . Over the southwest brink, a wide stream of glistening lava is sluggishly flowing in the direction of the Ka'u Desert, not with the spectacular cascading torrents of the southeastern flows of last March, but with a steady, stealthy gliding, which gains ground slowly at its face, but which piles up into tremendous masses from its source forward. Upon this southwestern side there is no longer any indication of the Hale-mau-mau Pit."

A vivid description is given of the lava plateau which has been heaped up over the former pit; "it is in reality a vast tilted roof which has been built entirely over Hale-mau-mau; broken in four or five places by almost circular spatter-walls erected thereon, within whose circumference are lakes, fountaining heavily and flinging molten torrents outward over the walls to flow onward over the surface of the roof."

The effect is a peculiar one, for as the lava seeks the hollows and fills up irregularities, it is producing an almost perfectly smooth sweep of floor over a mile in circumference. This condition will be stable for several hours, and then, as though the superincumbent weight had become insupportable, vast sections of the plateau will sink inward, releasing from beneath gigantic torrents of crimson and orange liquid lava, which surge upward and roar away over the adjacent surfaces, causing them in their turn to collapse and provide more pyrotechnics upon a tremendous scale, and repeating the process over and over again.

"Were it not for the fact that the southwestern overflow is following *a most unusual process* of damming itself back in walls of its own building as it advances," states Mr. Norton, "we should be witnessing such a torrential discharge of lava as *has never been seen* at Kilauea within the memory of man."

It is becoming increasingly evident that the present Hale-mau-mau rise is the usual equinoctial rise upon a greatly magnified scale, due partly to the abnormal squeezing of the Hawaiian fissure system, and to the unusual smallness of the previous fall after the last solstice, when the lava column withdrew little more than a hundred feet.

Starting its upward movement from a mean level some two hundred feet *above* the normal, it is only natural that the column should have reached the pit-rim a full month earlier than was anticipated. Since the column will, in the ordinary course of events, continue to rise until the time of the equinox in March next, the overflow may be expected to then attain an unprecedented magnitude.

A PROPOSED AMERICAN SOCIETY OF MAMMALOGISTS

A COMMITTEE of representative American mammalogists, including men from different parts of the country in its membership, has recently been at work on plans to organize a society for the promotion of interest in the study of mammalogy. It is intended that the society shall devote itself to the subject in a broad way, including investigations of habits, life histories, evolution and ecology. The plans call for the publication of a journal in which both popular and technical matter will be presented, for holding meetings both general and sectional, aiding research, and engaging in such other activities as may be deemed expedient. It is hoped to secure the active participation of all interested. The organization meeting will be held at the New National Museum, Washington, D. C., April 3 and 4, 1919, sessions commencing at 10:00 A.M. and 2:00 P.M. No program of papers has been planned for this meeting. The organization committee includes the following: Hartley H. T. Jackson, Chairman, U. S. Biological Survey; Walter P. Taylor, Secretary, U. S. Biological Survey; Glover M. Allen, Boston Society of Natural History; J. A. Allen, American Museum of Natural History; Joseph Grinnell, University of California; N. Hollister, National Zoological Park; Arthur H. Howell, U. S. Biological Survey; Wilfred H. Osgood, Field Museum of Natural History; Edward A. Preble, U. S. Biological Survey; Witmer Stone, Academy of Natural Sciences of Philadelphia. Further information will be furnished by either the chairman or the secretary, to whom applications for charter membership should be transmitted.

SCIENTIFIC NOTES AND NEWS

THE American Institute of Mining Engineers at its meeting in New York on Febru-

ary 18 elected as president Horace V. Winchell, of Minneapolis, Minnesota. Other officers elected were as follows: *Vice-presidents:* Edwin Ludlow, Lansford, Pa.; A. R. Ledoux, New York, N. Y. *Directors:* J. V. W. Reynders, New York, N. Y.; George D. Barron, Rye, N. Y.; Charles F. Rand, New York, N. Y.; Louis S. Crates, Ray, Arizona; Stanley A. Easton, Kellogg, Idaho. A memorial meeting was held during the afternoon for members who fell in service. The number of those who served in the Army or Navy reached some eight hundred, and information had been received of the death of twenty-five of these.

Dr. J. F. Abbott, professor of zoology in the University of Missouri, has been appointed commercial attaché to the American Embassy at Tokyo.

Professor E. C. Franklin returned to Stanford University, California, in December, after spending the greater part of the past year in research on the synthetic process for the fixation of nitrogen.

Dr. Chas. H. Herty, editor of the *Journal of Industrial Chemistry,* has been appointed chairman of a committee of the American Chemical Society on an Institute for Drug Research.

At their last meeting, held February 3, the trustees of The American Museum of Natural History elected Mr. Herbert L. Bridgman of New York an honorary fellow, in recognition of his valuable assistance rendered by service on a number of its most important exploration committees, and in special acknowledgment of his contribution to the advancement of science and education through his writings in the public press.

Dr. Louis Blaringhem, professor of agricultural biology at the Sorbonne, has been appointed exchange professor at Harvard University for 1918–19. Professor Blaringhem's term of service will fall in the second half-year.

The Janssen Prize of the Astronomical Society of France has been awarded to M. G. Raymond.

Sir Aubrey Strahan, director of the English Geological Survey, has been elected an honorary member of the British Institution of Petroleum Technologists.

Dr. G. Grandidier has been elected secretary of the French Geographical Society to fill the vacancy caused by the death of Baron Hulot.

Mr. J. C. Hostetter, of the geophysical laboratory, Carnegie Institution, has returned to Washington after a year's absence in charge of optical glass manufacture at Pittsburgh.

Dr. L. O. Grondahl, after having been commissioned a captain in the Ordnance Corps of the Army and having spent the last eighteen months in work on one of the problems of the Naval Consulting Board, has returned to his position as associate professor of physics, Carnegie Institute of Technology.

After twenty-one years of service in the Detroit high school and the junior college, Mr. Louis Murbach has resigned his position as head of the department of biology. His permanent address will be Castleton, Vt. He is succeeded by Mr. Norman Stoll, M.S., who besides high school experience has lately been teaching assistant in the department of zoology in the University of Michigan.

Major Edwin A. Ziegler, who had charge of the courses in orientation at the Coast Artillery School, Fort Monroe, Virginia, has resumed his work as director of the Pennsylvania State School of Forestry at Mount Alto.

Professor J. S. Illick, of the Pennsylvania State School of Forestry, has been appointed chief of the Bureau of Sylviculture in the Pennsylvania Department of Forestry.

Lewis A. Zimm has accepted an appointment as extension forester for Georgia and is connected with the Georgia State College of Agriculture. Mr. Zimm has been instructor in forestry and plant pathology at Cornell University and spent a season in dendropathological field work under Dr. Meinicke on the Pacific coast prior to his being commissioned in the army.

Dr. H. N. Holmes, head of the department of chemistry at Oberlin College, has finished a special piece of investigation connected with

the perfecting of gas masks. He has been appointed by the National Research Council member of a committee of four, known as the sub-committee on colloids.

PROFESSOR H. BURGER, of Amsterdam, has been elected corresponding member of the laryngological section of the Royal Society of Medicine at London.

MR. W. BOYD CAMPBELL, assistant superintendent at the forest products laboratory, McGill University, Montreal, is now in charge of the chemical engineering work of the Process Engineers Limited Montreal.

THE fourth lecture of Harvey Society will be given by Dr. Frederic S. Lee on "Industrial Fatigue" at the New York Academy of Medicine on Saturday evening, March 1.

PROFESSOR ROBERT F. GRIGGS, director of the National Geographical Society Katmai Expeditions, delivered an address on "Katmai and ten Thousand Smokes" before the Washington Academy of Sciences on February 18.

DR. ALEŠ HRDLICKA, curator of the division of physical anthropology, United States National Museum, will give four lectures on "The Origin and Antiquity of the American Indian" at the Wagner Free Institute of Science. The lectures, which are in the Richard B. Westbrook foundation, will be given on March 8, 15, 22 and 29.

SIR R. H. INGLIS PALGRAVE, distinguished for his work on economics and statistics died on January 25 at the age of ninety-one years.

DR. LOUIS-EDOUARD BUREAU, formerly profesor of botany at the Paris Museum of Natural History has died at the age of eighty-eight years.

DR. EUGENE PENARD, of Geneva, Switzerland, has nearly completed his great work on the Infusaria, on which he has been working for five years. He has material for two volumes of 850 pages each, but it will be necessary, on account of the cost of the publication, to condense it to a single volume of 650 pages. Dr. Penard is much occupied at the present time with his duties as a member of the commmission to administer relief to refugee Russians in Switzerland, under the American Red Cross.

UNIVERSITY AND EDUCATIONAL NEWS

A GROUP of alumni, headed by George P. Adamson, have completed the endowment of the Edward Hart fellowship at Lafayette College. The endowment is in the sum of $10,000, yielding $500 per annum, and is open to students of chemistry holding the bachelor's degree desiring to do research work in problems connected with viscous and plastic flow. The endowment was made in honor of Professor Edward Hart, who has completed forty years as professor of chemistry at Lafayette.

THE Women's College in Brown University received a gift of $50,000 to be used for a new dormitory.

DR. BENJAMIN IDE WHEELER has presented his resignation as president of the University of California.

PROFESSOR D. W. WORKING, of the Office of Farm Management of the U. S. Department of Agriculture, has accepted the positions of dean of the Arizona College of Agriculture and director of the Agricultural Experiment Station.

IN the absence on leave in Europe of Director H. Hayward, Professor A. E. Grantham, agronomist, has been appointed acting director of the Delaware College Agricultural Experiment Station beginning February 1. E. A. Hodson, of Cornell University, has been appointed assistant professor of agronomy.

DR. W. E. MILNE has been appointed professor of mathematics at the University of Oregon, to succeed Dr. R. M. Winger.

DISCUSSION AND CORRESPONDENCE

A STANDARD SCIENTIFIC ALPHABET

STANDARDIZING is one of the unending labors of science. By accurate standards scientists are able to test and prove, to plan intelligently, and to indicate precisely. Scientists substituted a simple and definite metric system for a great number of irregular and unrelated

systems of measurement of dimension, capacity, weight, etc. They substituted the exact centigrade thermometer with 100 degrees between freezing and boiling of water, for the system without absolute bases. They gave America and other countries an exact decimal system of moneys. They devised a decimal system for classification, for books, correspondence, etc.

But in the field of sound-notation or sound-representation, nothing comparable with the foregoing contributions to the world's progress and civilization has been done. For untold ages, the general capacity of the human vocal organs to make sounds has been the same. Each has the same provision of lips, teeth, tongue, palate, and the same provision as to lungs, larynx, windpipe, pharynx and nasal passages. Every normal person can, if trained, at least when young, make exactly the same vocal sounds as can any other normal person.

Ages ago men began to use these vocal sounds to express ideas; spoken languages resulted. Ages later "Cadmus, the Phenicians, or whoever it was," Egyptians, or others, struck upon the thought that a certain mark might stand for a certain sound. An alphabet was devised. Others developed, either offshoots of the first, or independently. To-day we have many alphabets. They were made originally for a particular language or dialect, and were limited to the sounds of that tongue; or they were borrowed from another people and but imperfectly suited the sounds of the borrowing language. None was made for all mankind; none was devised and none is adapted for the whole world. To-day in the new era after the war, the world needs an alphabet, a universal alphabet, a world-alphabet, a standard set of signs, characters or letters, full and complete, so that every sound used by any collection of human beings to indicate (alone or with other sounds) an idea, or to form a word of spoken language or dialect, shall be represented by one letter and only one letter; and so that every such letter shall stand for one and for only one sound.

The Roman alphabet which we and much of western and southern Europe uses, the Gothic, used (not to the exclusion of, but rather concurrently with Roman) by Germans and some Scandinavians, the Greek of Greece, etc., the Cyrillic of Russia and other peoples, the Gaelic, the Anglo-Saxon futhorc, the many cursive characters of Arabic, the Indian alphabets, the ideographs of the Orient, the special alphabets devised for aboriginal tribes of America, Africa and elsewhere—none meets the requirements set out above for a universal, world-alphabet. Such standard alphabet must be a scientific creation, or adaptation and adoption from present alphabets.

In the reorganization of the world at this time, a world conference of scholars and students, versed in many lines of art and science, should be held to devise and present a world-alphabet for consideration and adoption.

J. C. Ruppenthal

Washington, D. C.
(Russell, Kans.)

NONSILVERABLE CONTAINERS FOR SILVERING MIRRORS

Under this title[1] the writer has recently called attention to the observation that certain samples of "granite ware" enamelled iron pans did not seem to attract silver, in the ordinary process of silvering glass mirrors. However, it was not intended to convey the idea that one would expect this to hold true as a general rule.

Just why these pans did not take on a coat of silver while certain white enamelled pans did receive a thick coat of silver, is not understood—as is true of a great many other phenomena observed in attempting to deposit silver chemically upon glass. For example, it has been found easier to silver optical (white crown) glass than a certain mirror made of ordinary plate glass. One concave mirror, which is made of ordinary glass, always shows a spot where the deposit of silver is different from the rest of the surface, even after making a special effort in polishing and cleaning the glass surface. Again, making a container by tying a rim of clean writing paper around the edge of a glass disk, good mirrors were pro-

[1] Science, 48, p. 345, 1918.

duced; but no silver was deposited on the paper. Washing the glass with a certain kind of soap appears to interfere with the silvering process; while another kind of soap seems to be as effective as caustic potash.

Using a glass container, partially filled with the silvering solution, then (after the deposition of silver had started) filling the container with solution it was found on completion of the operation, that but little silver had deposited on the upper half of the container. The line of demarcation was sharp, just as though, once the deposition of silver had begun, the metal was attracted more readily to that part, of the receptacle. To conclude, it seems worth while to find a container that will not attract silver.

W. W. COBLENTZ

WASHINGTON, D. C.,
January 6, 1919

SYSTEMATIC PAPERS PUBLISHED IN THE GERMAN LANGUAGE

MY friend, Dr. W. T. Holland, has sent me copies of his article on the above subject in SCIENCE of November 8.

We are all agreed in our wish for the advance of knowledge and that the "eternal verities" are the only thing that will count in the long run; but in zoology the Russian, Hungarian, Japanese and other languages have never been recognized and I can not think that Dr. Holland himself would recognize descriptions published in the language he cites—Choctau. German is, without doubt, a barbarous language only just emerging from the stage of the primitive Gothic character, and I venture to suggest that it would be to the advantage of science to treat it as such from the date August 1, 1914. The science of botany is in many ways in advance of zoology. At the Botanical Congress at Vienna in 1905 men of Russian and various other nationalities objected to their languages not being recognized in science and it was found that the only method of arriving at an agreement was to insist on a Latin diagnosis being added in systematic papers in all languages, and this was agreed to. This regulation, though it has obvious disadvantages, may be found necessary in zoology also, the only alternative that I can see being descriptions in either English or French, the language of diplomacy. In recent Japanese works on entomology an English description is always added to the Japanese text.

G. F. HAMPSON

62 STANHOPE GARDENS,
LONDON, S.W.

SCIENTIFIC BOOKS

A Synopsis of the Bats of California. By HILDA WOOD GRINNELL. University of California Publications in Zoology, Vol. 17, No. 12, pp. 223–404, pls. 14–24, 24 figs. in text. January 31, 1918.

This work constitutes a notable contribution to the literature of California mammalogy and is characterized by the minute detail, thoroughness of treatment, and painstaking accuracy which one has come to expect in the publications of the Museum of Vertebrate Zoology of the University of California.

Following the introduction, the treatment is taken up under main headings as follows: Senses of bats, habits, voice, enemies, economic value, origin, geographic distribution, dentition, coloration, age-variation, sexual variation, nomenclature, classification, keys for identification, and table of comparative measurements, followed by treatment of the thirty-one forms belonging to eleven genera and three families of bats represented within the geographic limits of California.

Under each specific or subspecific heading appears a full annotated synonymy embracing the nomenclatural changes leading up to the name in current use and all references to the form as occurring in California. The particular species or subspecies is then discussed under the headings, diagnosis, description (including head, limbs and membranes, pelage, color, skull, teeth, measurements), synonymy and history, distribution, specimens examined, and finally natural history.

Data on senses and habits of bats are presented, chiefly compiled from the work of Halm, Ackert, Merriam, Miller, Howell and others, but supplemented by original observa-

tions by the author and her associates. In connection with habits the author states (p. 232) "The habits of our California bats are, unfortunately, but little known. The time and extent of the breeding season, migration and hibernation, the choice of diurnal retreats, and favorite feeding grounds, the methods of securing and devouring prey, the nature of the food, the economic value of bats—these are only a few of the many points on which data are as yet almost wholly lacking."

In connection with breeding habits and enemies, no mention is made of the interesting difference in numbers of young between crevice-inhabiting bats and certain tree-inhabiting species. As is well known *Nycteris cinereus* and *Nycteris borealis,* tree inhabiting forms, have from two to four young, while crevice and cave-inhabiting species have only one or two. As implied by Nelson[1] this seems to be due to the increased risk from the habitat relations of the tree-living forms. An augmented mortality probably results also from the migratory habit of *Nycteris.*

That bats deserve protection for their yeoman service in destroying insects goes without saying. It seems probable, however, that Campbell's estimate of the value of their work in Texas in destroying mosquitoes and thereby reducing malaria, quoted by Mrs. Grinnell on pages 238–239, is exaggerated. Stomach examinations of *Nyctinomus mexicanus* have shown many insects, but mosquitoes only rarely.[2]

The most adequate statement regarding the relation of bats to geographical distribution which has been seen by the reviewer is made by Mrs. Grinnell (pp. 242, 243), who emphasizes the fact that "conditions of temperature and humidity limit the distribution of bats as strictly as they do that of other groups of mammals. In California there is not a single obvious barrier to the distribution of any species of bat; yet not one of the thirty-one forms inhabiting the state has been found to be distributed uniformly throughout the entire area."

A study of the dates of capture of *Nycteris borealis teliotis* leads, according to our author, to the inference that "the sexes separate during the summer months, the females remaining in the Lower Sonoran zone, while the males migrate into the Upper Sonoran and Transition zones (p. 326)." After reference to a similar withdrawal from the breeding grounds by the males of certain species of birds, Mrs. Grinnell suggests that "Their departure relieves congestion in the nesting area and leaves a greater food supply for the females and young." In the case of red bats in California, however, one can not readily concede the implications either of congestion in their breeding area or the necessity, with an insectivorous species of the sort, for a greater food supply in summer; but the discovery of the separation of the sexes as noted, if confirmed with the acquisition of more material, is most interesting whatever its explanation.

There are no descriptions of new forms. New locality records are published for nearly every California species and subspecies. Subspecies *altipetens* H. W. Grinnell of *Myotis* is here referred to species *lucifugus* rather than *yumanensis* as originally described. *Myotis lucifugus alascensis* Miller is for the first time recorded from California, a specimen having been taken at Eureka. Definite records of occurrence in California of *Myotis lucifugus interior* Miller are also published for the first time. *Antrozous pacificus* Merriam is accorded full specific rank instead of being regarded as a subspecies of *pallidus.*

The paper is generously illustrated with half tones and maps. Life studies of several species appear, and comparisons of dorsal and lateral outlines of bat crania are made possible by an extensive series of enlargements. A few well executed line drawings show particular external characteristics of bats, and clear maps indicate the geographic ranges within California of all the forms considered.

[1] *National Geographic Magazine,* May, 1918, p. 491.

[2] See E. W. Nelson, *National Geographic Magazine,* May, 1918, p. 492.

A carefully prepared bibliography of 110 titles furnishes a ready key to the most important contributions to the literature.

One can but wish that the policy of the editors of the University of California Publications in Zoology provided for an index to individual papers, at least of the size of this one. This book, with no index, will probably be used as a separate publication by ten persons to one who will ever have occasion to consult it as bound in the volumes of the Publications in Zoology.

WALTER P. TAYLOR
BUREAU OF BIOLOGICAL SURVEY

BOTANICAL ABSTRACTS

THE plan of organization of a permanent board of control of botanical abstracts as outlined in the columns of this journal,[1] was effected at the Southern Hotel in Baltimore, on December 29, 1918. A joint meeting was held of the members of the temporary board, of the permanent board and of the board of editors at which was discussed many of the problems arising in connection with the undertaking. The following persons were present: A. F. Blakeslee, H. C. Cowles, B. M. Davis, B. M. Duggar, C. S. Gager, J. M. Greenman, A. S. Hitchcock, O. E. Jennings, B. E. Livingston, D. T. McDougal, Geo. E. Nichols, E. W. Olive, D. Reddick, J. R. Schramm and E. W. Sinnott.

The permanent board of control, which consists of two representatives elected from each of the allied societies, began functioning at this meeting. Some societies have been unable to hold a meeting at which representatives could be elected so that the following list represents the board as at present constituted:[2] American Association for the Advancement of Science, Section G, B. E. Livingston (4), A. F. Blakeslee (2); Botanical Society of America, Physiological Section, B. M. Duggar (4), W. J. Osterhout (2), Morphological Section, B. M. Davis (4), R. A. Harper (2), Systematic Section, J. H. Barnhart (4), A. S. Hitchcock (2); American Society of Naturalists, E. M. East (4), J. Arthur Harris (2); Ecological Society of America, Forrest Shreve (4), Geo. H. Nichols (2); American Phytopathological Society, D. Reddick (4), C. L. Shear (2); Paleontological Society of America, E. W. Berry (4), F. H. Knowlton (2); Society of American Foresters, J. S. Illick (4), Barrington Moore (2); Society for Horticultural Science.

The following actions were taken. They do not constitute the exact minutes of the meeting but are a codification of them with the omission of matters of ephemeral consequence. The temporary board of control was called to order at nine o'clock. It was voted unanimously that retiring members of the permanent board of control be not eligible for immediate reelection. The permanent board was thereupon organized with Donald Reddick and J. R. Schramm as temporary chairman and secretary, respectively.

Vote by ballot resulted in the election of Donald Reddick as chairman of the board of control of botanical abstracts. It was voted that an executive committee of five including the chairman be named by the chair, in consultation with the secretary. Drs. Harper, Livingston, Nichols and Shear were named. It was voted that the executive committee act as a committee on policy and make recommendations to the board of control prior to the annual meetings. Also that it attend to all *ad interim* business not involving change of policy.

The executive committee was given instructions as follows: (1) To incorporate the board of control of botanical abstracts; (2) to close a five-year printing and publishing contract with the Williams & Wilkins Co. of Baltimore; (3) to select editors for the sections for 1919, including those sections not now provided for; (4) to study and make a report at the next annual meeting on the arrangement of sections present within a fortnight to the botanical representative of the committee on grants of with reference to mycology; (5) to prepare and

[1] June 7, 1918, p. 558.

[2] The term of some members expires in two years and of others in four years, as indicated, but at the meetings of December, 1920, a new member will be elected by each of the participating societies for a term of four years, and such elections will be biennial thereafter.

the American Association for the Advancement of Science an application for a grant of funds. (Pursuant to an action taken by botanists at the dinner for all botanists.)

It was voted that the executive committee be informed that it is the consensus of opinion of the group present that the sections "bacteriology" and "cytology" be abandoned with the definite understanding that abstracts of articles in these fields be cared for by the other sections.

It was voted to appoint a committee, not confined to the board, whose duty it should be to prepare a list of all serials containing material to be abstracted for *Botanical Abstracts* and to appoint collaborators for such serials. J. R. Schramm was appointed chairman of the committee with power to select other members.

It was voted that the matter of including abstracts of zoological literature in *Botanical Abstracts* be left for the present to the discretion of the several editors, and that it be suggested to the committee that it limit its lists largely to plant literature.

Adjournment taken at three o'clock.

J. R. Schramm,
Temporary Secretary of the Board of Control of Bot. Absts.

SPECIAL ARTICLES

NON-SPECIFIC PROTEIN ANTIGENS PREPARED FROM SHATTERED HEMO-PROTEINS

For some time past I have been in quest of new methods to aid in combating various infections. I have tried out inhalations of several gases, hypodermic and intravenous injections of several salts and bases and acids, with practical negative results. However, recently I have obtained some very promising results while working upon the following hypothesis: In the blood and the blood-forming organs one may find the various compounds from which the building stones from which the various anti-bodies are formed during the process of active immunity to infection. Therefore the blood, or blood-forming organs may be the best material in which to find chemical compounds which may be isolated and used artificially to help the body resist infection. The blood in all probability contains the compounds which, when broken down to just the proper state of division, would yield a large number of proteins of relatively small molecular weight, which might act as antigens when introduced into the blood stream. The number and variety of these shattered products of blood digestion are doubtless very great, and some of them might well do the work of an antigen for almost any infection. In short, if the blood were properly digested and the various fragments of the digested blood tried out, it might be possible to find compounds which would not act harmfully in any way but would act as antigens in a great variety of infections.

In order to shatter the protein of the blood without destroying the particles it seemed best to employ no strong acids nor strong alkalis, nor any alcohol; but instead to use natural digestive enzymes. As a preliminary experiment I used Witte's peptone as a source for obtaining these shattered proteins. This worked very well, for Witte's peptone, being made from peptic digestion of blood fibrin proved to be very rich in these protein bodies. After trying Witte's peptone I prepared my own peptone and from that prepared my protein, from the fibrin of ox-blood by digestion with hydrochloric acid and pepsin. This method seems to be satisfactory. After the mixture of protein is prepared, it is separated into various parts such as primary and secondary proteoses and peptones. A mixture of secondary proteoses constitutes the protein we have used. The protein fractions were separated by precipitation with ammonium sulphate, the lower fractions being used, the other fractions rejected.

After long and careful testing on animals I have been using the protein in collaboration with clinical men on several different infections. Although the clinical side of the work has been going forward for over a year, in most diseases we are still far from a definite conclusion. However, Dr. Stanton and myself working on acute and chronic arthritis have found out that the protein is a very powerful remedy. It has given uniformly successful results on almost one hundred cases. Some of these were acute and others were chronic cases of years

standing. In collaboration with some of my pupils and other clinical men we have tested the protein in various streptococcus infections with good clinical results. More recently we have been extending the work to influenza, tuberculosis, pneumonia and all kinds of infections.

We do not yet know the exact compounds contained in the mixture of proteins which we have been using. There seems to be reason to believe that it is a rich mixture of many different individual proteins. We are making an effort to identify them as far as possible. It may be that the complexity of the mixture is the source of its power in so many different infections. We are also pursuing the inquiry as to the various possibilities of preparing other proteins from blood producing tissues and testing out on those infections which do not yield to the present protein.

I hope soon to publish a more complete account of the chemical work concerned with this problem and also of the clinical results obtained.

CLYDE BROOKS
THE OHIO STATE UNIVERSITY

A BIO-CHEMICAL THEORY OF THE ORIGIN OF INDIANAITE

INDIANAITE is the name applied to a variety of halloysite (Dana) by E. T. Cox.[1] It is a white mineral of porcelain-like appearance occurring in Indiana in beds varying in thickness from a few inches to eleven feet in rocks of the Mississippian and Pennsylvanian groups. Leo Lesquereux suggested that it had been formed by the burning out of a bed of coal.[2] Cox[3] advanced the theory that the Indianaite had been formed by the weathering and dissolution of a bed of limestone.

From studies in the field and laboratory the writer is convinced that the origin is due to bio-chemical action. Briefly stated the process is as follows: Shales containing pyrite are weathered and sulphuric acid is produced. The sulphuric acid attacks the clay forming aluminum sulphate. Sulphur bacteria absorb the soluble alum and rob it of its sulphur, secreting the aluminium in the form of a hydrated aluminium silicate which by a partial dehydration is rendered insoluble thus forming Indianaite. The writer has isolated the bacteria and finds them to be similar in appearance to *Beggiatoa alba*. That these microorganisms are influential in the origin of the Indianaite the writer believes he has demonstrated by experiments in the laboratory.

WILLIAM N. LOGAN
INDIANA UNIVERSITY

[1] See 6th Ann. Rept. Geol. Sur. Indiana, p. 15.

[2] See Rept. of a Geol. Reconn. of Indiana, 1862, p. 320.

[3] *Loc. cit.*

THE AMERICAN PHYSICAL SOCIETY

THE ninety-fifth meeting (the twentieth annual meeting) of the American Physical Society was held at Johns Hopkins University, in Baltimore, Maryland, on December 27 and 28, 1918, in affiliation with Section B—Physics, of the American Association for the Advancement of Science. Professor Bumstead is now serving as scientific attaché to the American Embassy in London and his resignation as president of the society was accepted by the council on November 30, 1918. The vice-president, J. S. Ames, thus became acting-president, and he presided at the several sessions of the society and the council. The maximum attendance at the technical meetings was about one hundred, while eighty-eight members and visitors were present at the time of the business session.

On the afternoon of December 27 there were two sessions under the auspices of Section B, the presiding officer being the vice-president and chairman of the section, Major G. F. Hull. At 2 o'clock, P.M., the retiring vice-president and chairman, W. J. Humphreys, gave an address on "Some recent contributions to the physics of the air." At five o'clock, P.M., Dr. George E. Hale gave an address before the entire association on "The National Research Council."

The annual business meeting was held at eleven o'clock, A.M., on December 28, 1918. The revised form of the constitution and by-laws was unanimously adopted by letter ballot. The amendments do not alter the intent or purpose of the constitution in the old form, except in one respect: the managing editor is made a member, *ex-officio*, of the council. The amended constitution will be published in the next printed list of members.

The following officers were elected for the year 1919:

President—J. S. Ames.

Vice-president—W. C. Sabine.

Secretary—D. C. Miller.

Treasurer—G. B. Pegram.

Members of the Council (four year term)—G. K. Burgess, J. C. McLennon.

Member of the Council (one year, unexpired term)—Max Mason.

Members of the Board of Editors of the Physical Review—Henry Crew, L. V. King, H. S. Uhler.

Colonel Millikan explained the purposes of the Smith-Howard Bill now before Congress, authorizing federal cooperation with the states for the promotion of engineering and industrial research. After general discussion it was unanimously voted that the American Physical Society favors federal aid and cooperation with the several states in support of research in science and engineering and in industrial research. The society favors the creation of boards of eminent scientists and engineers within each state for the administration of the funds appropriated for all research within the state.

At the morning sessions of December 27 and 28, 1918, thirty papers were presented as follows, four being read by title:

The unique system of units: W. W. Strong.

A simple stretched wire dilatometer: Arthur W. Gray.

Monochromatic and neutral tint screens in optical pyrometry: W. E. Forsythe.

The temperature, pressure, and density of the atmosphere in the region of northern France: W. J. Humphreys.

Refinements in spherometry: G. W. Moffitt.

A new type of hot wire anemometer: T. S. Taylor.

The linear thermal expansion of glass at high temperatures: C. G. Peters.

Some characteristics of glasses in the annealing range: A. Q. Tool and J. Valasek.

Striæ in optical glass: L. E. Dodd and A. R. Payne.

Preliminary determination of the thermal expansion of molybdenum: Lloyd W. Schadd and Peter Hidnert.

On the characteristics of electrically operated tuning forks: H. M. Dadourian. (Read by title.)

Ionization and resonance potentials for electrons in vapors of arsenic, rubidium and cæsium: Paul D. Foote, O. Rognley and F. L. Mohler.

Absorption coefficient of the penetrating radiation: Oliver H. Gish.

Photoelectric sensitivity vs. current rectification in molybdenite: W. W. Coblentz and Louise S. McDowell.

A device for the automatic registration of the α- and β-particles and γ-ray pulses: Alois F. Kovarik.

Note on the distribution of energy in the visible spectrum of a cylindrical acetylene flame: Edward P. Hyde, W. E. Forsythe and F. E. Cady.

Preliminary note on the luminescence of the rare earths: E. L. Nichols, D. T. Wilber and F. G. Wick.

On the critical absorption frequencies of chemical elements of high atomic numbers: William Duane and Takio Shimizu.

Some interesting results of eclipse magnetic observations: L. A. Bauer.

The minimum temperature at the base of the stratosphere: W. J. Humphreys. (Read by title.)

Why clouds never form in the stratosphere: W. J. Humphreys. (Read by title.)

Speeds in signaling by the use of light: W. E. Forsythe.

Thermal conductivity of various materials: T. S. Taylor.

Further observations on the production of metallic spectra by cathode luminescence: Edna Carter and Arthur S. King.

Effect of crystal structure upon photoelectric sensitivity: W. W. Coblentz. (Read by title.)

A mechanically blown wind instrument: A. G. Webster.

The dynamics of the rifle fired at the shoulder: A. G. Webster.

Interior ballistics, by a new gun indicator: A. G. Webster.

Residual gases in highly exhausted glass bulbs: J. F. Shrader.

Silvering quartz fibers by cathodic sputtering: J. F. Shrader.

Dayton C. Miller,
Secretary

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